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HETERODERA RADICICOLA ATTACKING THE CANADA
THISTLE

IN addition to the large number of plants known to be attacked by *Heterodera radicicola*, the writer has recently had occasion to find it infesting a new host—the roots of the Canada thistle, *Cirsium arvense*.

On December 10, 1913, the writer noticed the first indications of the root knot, occurring on tomato plants. This crop was being grown in one of the greenhouses belonging to the Department of Horticulture. On April 28, 1914, the plants were removed on account of their unproductiveness. Many of the plants, at the time of removal, showed their entire root system infested and destroyed by this eel-worm.

On April 4, 1914, Mr. J. B. Poole, of the department of botany, called the writer's attention to nodules occurring on the roots of *Cirsium arvense*. These plants were growing in a separate greenhouse from the one in which the tomato plants had been growing. The knots were very numerous, varying in diameter from two to ten mm. Their presence on the thistle roots, however, did not seem to interfere with the growth of this weed to any appreciable extent. A microscopic examination showed that the roots were badly infested with a nematode, and it seemed apparently to be the same species which occurred on the tomato. Cross sections of nodules showed the egg-filled bodies of female nematodes scattered throughout the cortex of the root. Specimens were sent to the Bureau of Plant Industry, and the determination verified.

The soil used in the various greenhouses was obtained from a nearby woodlot, and was probably badly infested with *Heterodera radicicola* at the time it was placed in the benches.

The fact that this organism is capable of living in the roots of *Cirsium arvense* should warrant the necessity of placing additional precautionary stress upon the eradication and destruction of this weed. Care should be exercised in treating soils before using, if the above weed should occur in any of the central or southern states, for the winters are prob-

ably not severe enough to kill the eel-worm by freezing.

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AN AVALANCHE OF ROCKS

THE Cadillac Trail on Mount Desert is one of the most picturesque features of the island. It is near Otter Creek and one enters the trail from the shore road. The trail leads by a gentle ascent to an irregular line of massive rock fragments which have fallen from some preexisting precipice farther up the mountain side. The path runs through and under and over these titanic blocks, some of which must weigh hundreds if not thousands of tons. The blocks and fragments stand at all angles. I have had no opportunity to consult any book on the geology of the island, but a hasty examination of the region leads me to believe that this avalanche of rocks must have fallen from some precipice which had been undercut by the waves when the land was below sea level, as we know the whole New England coast has been elevated since the ice sheet retreated. The glacial clays with Arctic species of Mollusks, still living in Hudson Bay, are found from Danvers, Massachusetts, east to Lubec, Maine, and beyond, and indicate a former subsidence of the coast many feet below the level of the sea. Now if this event occurred at the time of this depression the material buried beneath these rocks would be of very great interest. If some large fragment rested on the parent ledge it could be tilted sufficiently by hydraulic jacks to enable one to gather the stuff beneath, and it might reveal the shells, diatoms, foraminifera, etc., living at the time of this catastrophe, and possibly the compressed vegetation might reveal important features also. The exploration could be made at a moderate expense and the conditions could be easily restored.

EDWARD S. MORSE

SCIENTIFIC BOOKS

Problems of Genetics. By WILLIAM BATESON.
Yale University Press. 1913. Pp. ix +
255, illustrated.

The Silliman lectures delivered at Yale by Professor Bateson in 1907, revised and published in 1913 under the title "Problems of Genetics," form one of the most stimulating and suggestive books for students of Evolution and Heredity which has appeared since the rediscovery of Mendel's law. Like other books by the same author, it is not designed for beginners but for actual workers in the field covered, and is no fit food for babes and sucklings. To the student familiar with current theories and lines of investigation concerning evolution the frank criticism of those theories and investigations will be especially valuable. Present knowledge is recognized to be imperfect and tentative, and while the author expresses an emphatic preference for certain views, he holds them without dogmatism or claim to finality, an attitude in scientific work which, with every advance, we need to remind ourselves of anew.

In his introduction, Bateson addresses himself to the old but unsolved question of the nature and origin of species, which he blames the discussion over natural selection for obscuring. He says: "In the enthusiasm with which evolutionary ideas were received the specificity of living things was almost forgotten. The exactitude with which the members of a species so often conform in the diagnostic, specific features passed out of account; and the scientific world by dwelling with a constant emphasis on the fact of variability, persuaded itself readily that species had after all been a mere figment of the human mind. Without presuming to declare what future research only can reveal, I anticipate that, when variation has been properly examined and the several kinds of variability have been successfully distinguished according to their respective natures, the result will render the natural definiteness of species increasingly apparent." Bateson rejects natural selection as a sufficient explanation of the origin of species, concluding that we are on safer ground "in regarding the fixity of our species as a property inherent in its own nature and constitution." He says: "As soon as it is realized how largely the phenomena of varia-

tion and stability must be an index of the internal constitution of organisms, and not mere consequences of their relations to the outer world, such phenomena acquire a new and more profound significance." In Chapters II.-IV., Bateson attempts a classification of variations along lines indicated in his "Materials" (1896). This may have been important historically in leading Bateson to his later views concerning the discontinuity of evolution, but to most readers will seem aside from the main discussion. Taking up in Chapter V. the mutation theory, he expresses the view that a new species originates in a changed "internal constitution of the organism," which with DeVries he believes to arise discontinuously. But the discontinuity, according to Bateson, is of Mendelian unit-characters or factors only. He explicitly rejects the DeVriesian idea of mutation involving a change in a whole group of characters simultaneously, and explains the peculiar genetic behavior of *Oenothera lamarckiana* as due to hybridization.

"The facts may, I think, fairly be summarized in the statement that species are, on the whole, distinct and not intergrading, and that the distinctions between them are usually such as might be caused by the presence, absence or inter-combination of groups of Mendelian factors; but that they are so caused the evidence is not yet sufficient to prove in more than a very few instances.

"The alternative, be it explicitly stated, is not to return to the view formerly so widely held, that the distinctions between species have arisen by the accumulation of minute or insensible differences. The further we proceed with our analyses the more inadequate and untenable does that conception of evolutionary change become. If the differences between species have not come about by the addition or loss of factors one at a time, then we must suppose that the changes have been effected by even larger steps, and variations, including groups of characters, must be invoked.

"That changes of this latter order are really those by which species arise is the view

with which DeVries has now made us familiar by his writings on the mutation theory. In so far as mutations may consist in meristic changes of many kinds and in the loss of factors, it is unnecessary to repeat that we have abundant evidence of their frequent occurrence. That they may also more rarely occur by the addition of a factor we are, I think, compelled to believe, though as yet the evidence is almost entirely circumstantial rather than direct. The evidence for the occurrence of those mutations of higher order, by which new species characterized by several distinct features are created, is far less strong, and after the best study of the records which I have been able to make, I find myself unconvinced. The facts alleged appear capable of other interpretations.

"DeVries found, as is well known, that *Oenothera lamarckiana* gives off plants unlike itself. These mutational forms are of several distinct and recognizable types which recur, and several of them breed true from their first appearance. The obvious difficulty, which in my judgment should make us unwilling at present to accept these occurrences as proof of the genesis of new species by mutation, is that we have as yet no certainty that the appearance of the new forms is not an effect of the recombination of factors, such as is to be seen in so many generations of plants derived from a cross involving many genetic elements."

The phenomenon of twin-hybrids he does not consider satisfactory evidence of *group inheritance* of characters, but to have its best explanation "in the well ascertained fact that the male and female germ-cells of the same individual may be quite different." By this is meant that the pollen and ovules of the same plant may transmit different qualities respectively.

In order to throw light on the question whether species originate discontinuously or not, Bateson discusses, in successive chapters, "Variation and Locality," "Overlapping Forms," and "Climatic Varieties," bringing together a great amount of illustrative material partly of his own collecting, partly the

work of others. Special attention is given to the nearly related species of North American flickers and of warblers, which are illustrated by two beautiful colored plates. These cases have been selected because they seem to show specific differences consisting in Mendelizing unit characters. Even in these cases, however, the existence of such unit characters is inferential rather than demonstrated and actual experimental work on the crossing of wild species of birds, such as pheasants studied by Ghigi and Phillips, and pigeons studied by Whitman, though it reveals the frequent occurrence of unit character differences between one domesticated variety and an original wild species, rarely shows the existence of such differences between one wild species and another. Even granting that unit character differences occasionally occur between one wild species and another (as I believe they do), it may well be that such differences, though striking, are not the most important or essential ones. As Bateson himself says in another connection (p. 184), "It seems in the highest degree unlikely that the outward and perceptible character or characters which we recognize as differentiating the race should be the actual features which contribute effectively to that result." If an extensive survey were made of related wild species of birds or mammals, I suspect that it would be found that the discoverable differences in a majority of cases consist in quantitative differences in characters, rather than in presence and absences of striking single characters. Consider for example the genus *Mus*. The black rat, *M. rattus*, as the experiments of Morgan and Bonhote have shown, is distinguished from *M. alexandrinus* by a single unit character difference. The two cross freely and Mendelize on crossing, but without producing any new third form, so far as we know at present. The color difference between them is a very striking one, but it appears to be the only existing difference. The one might be described as a color variety of the other.

Compare now *M. alexandrinus* with *M. norvegicus*. The two are very similar in appearance. Only quantitative differences in

size and proportions of parts serve to distinguish them. Yet they are so distinct genetically that they never cross naturally and all attempts to cross them artificially have thus far failed. No one would, I think, advocate the idea that one had arisen from the other by unit character variations, such as distinguish *M. rattus* from *M. alexandrinus* or striking tame varieties of the Norway rat from the wild species. The differences in these latter cases are unquestionable, and their genetic behavior clear, but if we call forms so distinguished *species* it is evident that we are applying the term on the basis of very different phenomena from those which serve to distinguish *M. alexandrinus* from *M. norvegicus* or *M. musculus*. In these cases the observable differences are quantitative and their genetic behavior unknown. There is small reason for considering them unit character differences. It is of course possible so to regard them if one conceives of unit characters or factors in such cases as very numerous and singly with small effect, in accordance with the principle of Nilsson-Ehle. But so to conceive of a unit character is to rob it entirely of that which the theory of discontinuity in the evolution of species requires and which Bateson seeks to establish. Multiple unit characters presenting an apparently continuous series would also have no selectional value superior to that of a *truly* continuous series of variations, the conception which Bateson combats.

Bateson devotes one of the most valuable chapters of his book to the subject of adaptation, without either reaching or attempting to reach any explanation of it. Indeed he rather deplores the fact that so much attention has been devoted to the adaptation of species before we have arrived at any clear notion as to what species are or how they arise. The chief value of Bateson's discussion of this question lies in the destructive criticism which he offers of the attempted explanation of adaptation as a direct response of the organism perpetuated by heredity. He passes over the earlier discussions concerning the inheritance of acquired characters, but deals with its recent

vigorous renewal by Semon, who regards heredity as analogous with memory or habit. Bateson holds that an analogy with psychic phenomena is no explanation, among other reasons because the explanation is necessarily more complicated than the thing explained. The evidence on which Semon relies to establish the inheritance of acquired characters, Bateson deals with at some length. He shows the inadequacy of the oft-cited temperature experiments with lepidoptera to show either an increased variability due to experiment or its inheritance. The case of Schubeler's wheat adapting itself automatically to the shorter season of Norway is subjected to destructive criticism, as is also the case of Brown-Sequard's guinea-pigs, so often brought forward, so often shown to be of no consequence. Special attention is given to the recent work of Kammerer at Vienna upon salamanders, on which Semon places great reliance. By keeping land salamanders in water and vice versa, Kammerer claims to have modified the structure and habits of these animals permanently, the young inheriting the acquired modifications of the parents even when restored to normal conditions, and the inherited effect increasing from generation to generation upon continuation of the experimental conditions. Bateson shows that these extensive claims are based on wholly inadequate experiments, that the author is unable or unwilling to produce specimens of the modified structures which he claims to have obtained, and that unless his observations are independently confirmed it is "easier to believe that mistakes of observation or of interpretation have been made than that any genuine transmission of acquired characters has been witnessed."

"Meanwhile there is no denying that the origin of adaptational features is a very grave difficulty. With the lapse of time since evolutionary conceptions have become a universal subject of study that difficulty has, so far as I see, been in no wise diminished. But I find nothing in the evidence recently put forward which justifies departure from the agnostic position which most of us have felt obliged to assume."

A chapter of especial interest to Americans discusses "The Causes of Genetic Variation," for the work reviewed is to a considerable extent that of American biologists, who have attempted to produce and claim to have succeeded in producing heritable variations under controlled experimental conditions. The work of Woltereck in Germany has shown, according to Bateson, that the character of the food supplied to a parthenogenetic *Daphnia* affects the structure of her immediate offspring, but the effect does not persist further into subsequent generations. Hence there is no permanent racial influence. Tower, however, in potato-beetles, and MacDougal in *Raimannia* claim to have brought about permanent racial changes, the one by altering the temperature and humidity at which the parent beetles are kept, the other by injecting certain salt solutions into the ovaries of the parent plants. Bateson points out that neither of these important results has been independently confirmed by experiment, though this has been attempted by Compton with negative results in the case of *Raimannia*. After reviewing Tower's two principal papers and pointing out a number of inconsistencies, Bateson adds

"The hesitation which I had come to feel respecting these two publications of Tower's has been, I confess, increased by the appearance of a destructive criticism by Gortner who has examined the parts of Chapter III of Tower's book in which he discusses at some length the chemistry of the pigments in *Leptinotarsa* and other animals. As Gortner has shown, this discussion, though offered with every show of confidence, exhibits such elementary ignorance, both of the special subject and of chemistry in general, that it can not be taken into serious consideration."

Regarding MacDougal's work he says, emphasizing the need of repeating the experiment with *Raimannia*:

"He [MacDougal] adds that he is making similar experiments with some twenty genera; but what is more urgently needed is repeated confirmation of the original observation. When it has been shown that this mutation

can be produced with any regularity from a plant which does not otherwise produce it on normal self-fertilization, the enquiry may be profitably extended to other plants."

The net result of Bateson's discussion of the causes of genetic variation is negative. No means of controlling genetic variation has, he believes, yet been found.

A chapter dealing with The Sterility of Hybrids presents many interesting questions without answering any of them satisfactorily. Interspecific sterility is shown to be important in keeping species distinct, and it is suggested that in some cases at least it is connected with unit character inheritance, but beyond this point all is uncertainty.

In his concluding remarks, Bateson emphasizes the present partial and incomplete state of our knowledge of genetic problems and in particular of what a species really is. He expresses the conviction that it is not a mere arbitrary group of organisms, though to the systematists it can hardly be anything else. "Their business," says Bateson, "is purely that of the cataloguer, and beyond that they can not go. They will serve science best by giving names freely and by describing everything to which their successors may possibly want to refer, and generally by subdividing their material into as many species as they can induce any responsible society or journal to publish.

"As yet the genetic behavior of animals and plants has only been sampled. When the work has been done on a scale so large as to provide generalizations, we may be in a position to declare whether specific difference is or is not a physiological reality."

W. E. CASTLE

Vorträge über Deszendenztheorie. Von AUGUST WEISMANN. Dritte umgearbeitete Auflage. Jena, G. Fischer. 1913. Pp. xiv + 354, 3 pls., 137 figs. in text.

Mendel's Principles of Heredity. By W. BATESON. Cambridge, Eng., Univ. Press, and New York, G. P. Putnam's Sons. 1913. 3d Impression. Pp. xiv + 413, illustr.

These two books deal with the two most im-